thing apart from but are inseparably bound up with sensory complexes. It is probable that the difficulty or impossibility of the psychological analysis of emotional states is due to the fact that their synthesis is effected in the physiological field below the threshold of consciousness, so that consciousness can deal only with the net result of inherited physiological co-ordinations—a view which is, again, in full accord with Mr. Marshall's own conclusions.

We must pass over the interesting discussion of "a group of co-ordinated activities tending to bring about attraction of other individuals," which Mr. Marshall terms the art-impulse, artistic creation having this end in view, though not cognised as the end. His contention is to some extent corroborated by that thirst for appreciation and recognition which forms part of the artistic temperament, and it harmonises with many observations on animal activities.

Coming now to the field of æsthetics, the author seeks to find some criterion by which æsthetics may be differentiated from algedonics. The conclusion to which he is led is as follows:—"That object is to be considered beautiful which produces a psychosis that is permanently pleasurable in revival," while "that object is to be considered ugly which produces a psychosis that is permanently disagreeable in revival." Thus "only those pleasures are judged to be *æsthetic* which (relatively speaking) are permanently pleasurable in memory." We believe that the author is here on the road to, but falls somewhat short of, the true criterion of æsthetics. The key of the problem, we think, lies in the recognition of the algedonic tone of *perceived relations*. It is this superadded element which raises the algedonics of sensory experience to the level of æsthetics. The æsthetic effect of the geometrical tracery in the chapter-house of Wells Cathedral is due to the emotional tone associated with perceived relationships. And it is just because in memory the relationships with their emotional tone are more abiding than the sense-elements, that to be permanently pleasurable in revival becomes a criterion of æsthetics. This criterion is, however, secondary. The primary criterion is the perception of relations with its associated emotional tone.

It is difficult to do justice, in the short space which remains to us, to the author's views as to the physical basis of pleasure and pain. These primitive qualities of psychical states are conceived to be "determined by the relation between activity and capacity in the organs, the activities of which are concomitants of the psychoses involved." When an organ during rest has stored up energy, the response of the organ to stimulus is pleasurable. But when the organ is spurred to activity beyond the limits of its stored up energy, its functioning is painful. "Pleasure thus results when the balance is on the side of the energy given out, and pain when the balance is on the side of the energy received. Where the amounts received and given have equivalence, then we have the state of indifference." We have seen that Mr. Marshall does not accept the hypothesis that there are separate end-organs, nerve fibres, or cerebral centres for pleasure and pain. Unless, therefore, there is a qualitative difference in the impulses transmitted from an organ according as it is well-stored with energy or exhausted, a position which is hardly tenable, the algedonic tone must be due to quantitative difference—that is to say, difference in the intensity of stimulus. Hence it would be better, so far as the organ is concerned, to lay the primary stress on the intensity of stimulus therefrom, and to make the state of the organ a condition of this intensity. Mr. Marshall ought also, we think, to supplement his view by reference to the condition of the cerebral centre concerned. The condition of the centre is possibly of even greater importance than the condition of the organ from which afferent impulses are transmitted. We cannot, however, further discuss the question here, and must refer our readers to the author's own treatment of the question in the fourth and fifth chapters of his work.

Although we do not agree with all his conclusions, we have no hesitation in saying that the book is written in the right spirit and on right lines. Fully aware of the necessity for careful introspection, he sees that the results so reached must be correlated with the conclusions arrived at through the investigations of the physiologist. It is only where the two modes of investigation thus go hand in hand that progress in psychology can be secured.

C. Ll. M.

OUR BOOK SHELF.

Physiology Practicums. By Burt G. Wilder, Professor of Physiology, Vertebrate Zoology, and Neurology in Cornell University, U.S.A. (Published by the Author, 1893.)

THESE consist of a series of twenty-seven plates, with accompanying descriptions (large octavo), said by their originator to embody "explicit directions for examining portions of the cat, and the heart, eye, and brain of the sheep, as an aid in the study of elementary physiology." The author is well known in anatomical circles as the founder of a notoriously ambitious terminology, not wholly destitute of useful points. The present venture has furnished him a new peg upon which to hang this, and his title savours of the kind of treatment which the subject receives at his hands. Plate xviii. Fig. 19 (which deals with the "pelvic viscera, etc." [*sic*], of the female cat), and Plate xiii. Fig. 14 (which is said to represent the "head and neck of cat partly dissected"), may be taken as fair examples. With their faulty delineation of things which may be at once determined from descriptions alone, their ugly letters sprawling over them, and their apologetic descriptions, they are useless and uncalled for; and the matter is the more nauseous, as more than one finished anatomical treatise happens to deal with this animal. We put the plates down with the feeling that they are calculated to repel rather than encourage the student, and that although they may be of service in the work of the Cornell University, in connection with which they have arisen, there would be cause for alarm should they be adopted elsewhere.

The Fauna of British India, including Ceylon and Burmah. Published under the authority of the Secretary of State for India in Council. Edited by W. T. Blanford. "Moths." Vol. ii. By G. F. Hampson. Pp. xxii. 609, (325 woodcuts). (London: Taylor and Francis, 1894.)

THE second volume of Mr. Hampson's important work on the moths of India includes the *Arctiidæ*, *Agaristidæ*, and the bulk of the *Noctuidæ*, and considerably exceeds the first volume in bulk, 1545 species being described in vol.

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ii. as against 1158 in vol. i. Under *Arctiidæ* the author includes the following groups as sub-families, which have usually been treated as families by previous authors:—*Arctiinæ*, *Lithosiinæ*, *Nycteolinæ*, and *Nolinæ*. The *Agaristidæ* are a small family of handsome day-flying moths, and certainly look rather out of place in the position which they occupy in this book. The extensive family of *Noctuidæ* is divided into ten sub-families (*Trifinæ*, *Acontiinæ*, *Palindiinæ*, *Sarrothripinæ*, *Euteliinæ*, *Stictopterinæ*, *Gonopterinæ*, *Quadrifinæ*, *Focillinæ*, and *Deltoidinæ*), of which the two last are held over to the forthcoming third volume of the book.

Concerning the *Noctuidæ*, Mr. Hampson remarks, "The lowest forms are those of which the larvæ have five pairs of abdominal prolegs, and the perfect insects have vein 5 of the hind wing fully developed, and from the centre of the discocellulars, this ancestral form being only found in some *Deltoidinæ* and *Sarrothripinæ*."

As the plan of the second volume is identical with that of the first, which we had the pleasure of noticing in NATURE for February 23, 1893 (pp. 387–388), we need only add that there seems no falling off in its execution. It is hoped that the third volume, including the *Epicopiidæ*, *Uraniidæ*, *Epiplemidæ*, and *Geometridæ* will be completed in the course of the present year.

W. F. K.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Panmixia.

MR. ROMANES has requested those students of natural history who cannot accept the doctrine of Panmixia to show the error which they believe to lie in his reasoning. I therefore ask leave to explain why I am unable to accept either the first proposition put forward by Mr. Romanes in NATURE of to-day, or the doctrine itself. Mr. Romanes says:—

The survival-mean must (*on cessation of selection*) *fall to the birth-mean*, &c. This statement involves neglect of a way in which selection may, and often must, operate. A simple example will show this. The mean height of adult Englishmen is roughly 67½ inches; and if I offer to enroll in a regiment every adult Englishman who is more than 66 and less than 69 inches high, the mean height of my regiment will, as every statistician knows, be still 67½ inches, but I shall be obliged to reject more than half the population. A form of selection, involving the destruction of more than half the population, may therefore occur without affecting the mean value of the character selected. I hope shortly to publish evidence, based on the measurement of many thousands of animals of one species, at many stages of growth, showing that selection does in fact operate in this way in particular cases. That it must so operate in many cases is obvious from the fact that many wild animals remain for several generations without sensible change in their mean character. In these cases either selection acts as I suggest, or it is incapable of affecting a change in the mean, or it does not act at all.

The second and third propositions put forward by Mr. Romanes are not demonstrated by any statistics with which I am acquainted; and with regard to the extreme statement that "*any* failure in the perfection of hereditary transmission will be weeded out" by selection in a wild state, I would urge the need, which has lately been well pointed out by Bateson, of a *quantitative* measure of the efficiency of selection. The frequency of even considerable abnormalities in specialised organs of wild adult animals, of which so many admirable examples are described in Mr. Bateson's recent work on variation, show, if it needed showing, that natural selection is in most cases an imperfect agent in the adjustment of organisms.

But my main difficulty is that neither Mr. Romanes, nor Prof. Weismann, nor any other advocate of the doctrine, has shown that in some given case Panmixia does in fact occur, and that the results predicted are in fact produced. On the other hand, Mr. Galton has shown that civilised Englishmen are themselves in a condition of Panmixia, at least with respect to several characters, especially stature and the colour of the eyes. Now the mean stature of Englishmen is known to be slowly increasing, and there is no evidence of the disappearance of coloured eyes.

My objections to the position of Mr. Romanes and others are therefore two: first, that it is based on the assumption that selection, when acting on a species, must of necessity change the mean character of the species—an assumption incompatible with the maintenance of a species in a constant condition; and secondly, that in the only case which has been experimentally investigated, the consequences said to result from a condition of Panmixia do not, in fact, occur.

W. F. R. WELDON.

University College, London, April 26.

On Some Sources of Error in the Study of Drift.

As a general rule we may feel sure that the boulders scattered over the surface of a district which consists chiefly of boulder clay, have been derived from the underlying deposit. There are, however, some cases in which the inference is unsafe. For instance, the Thames now marks the southern limit of the glacial drift—a curious circumstance, and one of which a wholly satisfactory explanation has not been given. Many think that this sharp definition of the southern limit of the glacial drift is so improbable that they would fain attribute some deposits in North Kent to the glacial period, or at any rate would expect to find a few sporadic boulders stranded on the slopes of the North Downs; and there far-transported fragments do not unfrequently occur.

But there is this great source of error. All along the lower Thames barges carry refuse and rubbish of every description from London, and this is taken, such as it is, and laid on the adjoining lands.

So you find carried on, with road scrapings, fragments of every kind of road metal; with soil turned out in digging foundations, specimens of all the materials used for building; with the contents of middens, every variety of object of domestic use or ornament. It is marvellous what large lumps get on to the land in this way. When, then, anyone produces a specimen, even a large specimen eight or ten inches in diameter, and perhaps taken out of a deep loam, the evidence is rejected. The stone may have been carried on to the land with the manure, and the loam may in that district be quite recent rain-wash. It may be that some of them were really of glacial origin, but all are equally distrusted. Some of them certainly cannot be referred to ice action. I have seen large pieces of Napoleonite found on the surface in North Kent. By what accidents they came to be there we cannot tell, but we may, at any rate, acquit the ice of having had anything to do with the transport of that peculiar Corsican rock.

When walking along the base of a cliff of boulder clay, we may generally infer that the far-travelled boulders that lie at its base have just been washed out of it. In most cases they have been; but in some, and those often the cases in which it is of greatest consequence to have the origin of the boulders clearly established, we have another serious source of error, of which I have just seen a good example.

A Norwegian vessel, carrying timber from Christiansund to Boston, in Lincolnshire, ran aground and became a total wreck off Old Hunstanton last winter. I saw her in January. The vessel looked sound enough to a landsman's eye; but she was dismasted and gutted, and the salvage was on the sand dunes close by. About her a pool of varying breadth had been formed by the swirl of the water round the hull. The currents had been deflected by various circumstances here and there, as especially where a quantity of ballast had been thrown out. This consisted of large boulders of various kinds of gneiss and porphyry, and the weighty pile looked as if it were little affected by the currents of the incoming and receding tides.

In April, I visited the spot again, expecting to find that the boulders had been driven along the shore by the fierce storms which had raged along that coast since my previous visit, and intending to make note of their dispersal and the distance to which they had travelled. I found, however, that the keel and a portion of the lower part of the wreck remained, and that the surrounding pool was greatly deepened and extended.

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